OPEN LETTERS.

The Origin of Floral Structures.

I am always glad to receive criticisms of my theory from an opponent, but my reviewer, "R.," somewhat misrepresents me rather than criticizes, for he says (BOT. GAZ. xiii, 324): "The Darwinian theories of natural selection and of cross-fertilization are thus wholly repudiated." My exact words are (*Or. of Fl. Str.* p. 336), "I do not wish the reader to suppose that my theory is altogether in opposition to Mr. Darwin's." I by no means reject natural selection, and even give the result of an experiment showing how a hard Russian wheat when sown thickly with a soft English grain was "selected," as that only carried ears. I adopted the expression "constitutional selection" as best describing that kind. I recognize natural selection as a factor in several ways, but never as *a cause*, and so not having the importance which has generally been attached to it. None of my many reviewers have credited me with denying it, either in England or America, except "R." "R." quotes the Papilionaceæ as opposed to my theory, in having the standard larger than the anterior petals; but this agrees with well nigh all irregular flowers in which the stamens afford the landing place, *e. g.*, Pelargonium (except "the Scarlets"), Rhododendrons, horse-chestnut, Amaryllis, etc. I have attributed this result in part to atrophy of the anterior petals (p. 111) without precluding a certain amount of hypertrophy on the opposite (dorsal or posterior) side (p. 116). "R." must have overlooked what I have written on this as well as on the resupinate labellum of Orchis (p. 107). "R." assumes Verbascum to be a further advance of the Personales. I regard it as an ancestral form and as more nearly approaching the primitive and regular type of flower; for I know of no case where an irregular flower passes into a regular one *except in a pelorian condition* (see chap. xiv). This Verbascum most certainly is not. Why "R." calls zygomorphic types "ancient" does not appear. This is one difference of importance between our respective views, in that I ventured to offer my theory as suggestive or as a "working hypothesis" only (p. 3); on the other hand, "R." states his opinions in a very categorical manner, as if they were not open to doubt at all. Thus he says: "Although it is evident that natural selection must act"—why "evident"? why "must"? I agree with Prof. Huxley, who says that a scientist does not know the word "must." If "R." had pointed out *how* natural selection produces a combination of minute characters in all the organs of a flower, including the floral receptacle; and *all* in harmonious correlation with insect fertilization; I should have been glad to have read it (see p. 330) and his review would have been more satisfactory; but merely to say natural selection "must" have brought them about is neither an answer nor a criticism.

I can only add that I am extremely gratified to find that the views of such able naturalists as Prof. Packard, A. W. Hyatt and others to be thoroughly in accordance with my own. I had no conception that neo-Lamarckism was so widely sustained in the United States.

London, England. GEORGE HENSLOW.

I am satisfied to leave the reader with the context of Prof. Henslow's quotation: "Instead, therefore, of using this term (natural selection) as the cause of anything and everything, I prefer to attribute effects to hypertrophy, atrophy, resistance to strains, responsive action to irrita-

tions, and so on. If it be thought that natural selection somehow underlies all this, the reader is at liberty to substitute the phrase; but, I must confess, it conveys nothing definite to my mind, while the others undoubtedly do. I do not wish the reader to suppose that my theory is altogether (sic) in opposition to Mr. Darwin's; for it must not be forgotten that he himself laid great stress on the environment as a cause of variability upon which, when once brought about, natural selection could then act." I understand this to mean that he agrees with Mr. Darwin in ascribing effects to environment, but not in regard to natural selection. The fact that H. denies the advantage of cross-fertilization is sufficient ground for saying that he repudiates natural selection as an explanation of floral mechanisms. I regard natural selection, not as a cause of hypertrophies and atrophies, but as a cause of adaptations, the most important characteristics of organs and organisms. However, I think of natural selection not so much as a cause as a controller of causes. No doubt H. regards heredity as a cause. But heredity can only insure that a given generation shall resemble its progenitors. Natural selection determines who those progenitors shall be. In regard to other reviewers I quote from *Journ. Bot.* xxvi, 313: "Professor Henslow, for example, is a well-known upholder of the principle of evolution; but in the present work he vehemently combats two of the theories which are most closely associated with the great name of Darwin." H. regards a flower as a geologist would regard a hill, *i. e.*, as a resultant of all the forces which have been brought to bear upon it. But organisms resist or avoid the direct effects of their environment, being active in controlling their conditions, or in adjusting themselves to them. In opposition to this view, I hold that many highly specialized flowers, instead of developing to suit their principal visitors, have contracted the parts in front of the receptacle, excluding one set of visitors after another from the landing until the largest bees could only insert their tongues, *e. g.*, Trifolium pratense and Amphicarpæa monoica.

However, from the standpoint of pure Lamarckism, if we admit that insect contact has a given effect, I hold that the theory will not account for the facts of floral structure. If insects leave the perianth altogether, H. claims that the whole perianth atrophies. If they leave the perianth and light upon the stamens, the perianth atrophies below and hypertrophies above. In the case of Papilionaceæ, etc., I claim that at first direct insect contact was equally absent both above and below and had nothing to do with the reduction of one part or with the enlargement of the other. What he calls atrophy-hypertrophy are the things to be explained, and they can not furnish the explanation. The labellum of orchids is also against the theory. My view is that it was developed as a vexillum on the upper side of the flower, and that its enlargement instead of being a result of its use as a landing after inversion is rather the cause of the inversion. Moreover, in my neighborhood, Habenaria leucophæa, visited by hawk-moths, which suck without touching the labellum, has this part as well developed as in Orchis spectabilis, whose labellum forms a landing for humble-bees. What does insect contact have to do with the colored bracts of Euphorbia and Cornus florida, or with the neutral flowers of Hydrangea and Helianthus annuus?

I consider Verbascum a degradation rather than an advance of the Personales. I call the zygomorphic type of Personales ancient, because it is the type of the cohort. Campanula Americana is in the first stage of zygomorphy. The type of the genus Campanula is actinomorphic. But in the case of Verbascum the type not only of the great order of

Scrophulariaceæ, but also of the great cohort of Personales is zygomorphic. In the Campanula the irregularity is limited to the deflection of the style, while in Verbascum it involves both the stamens and petals. I suppose the type of Scrophulariaceæ to have been a flower with a tube long enough to cover the stamens so that insects could not light upon them, and so narrow as to crowd the stamens and style when they changed to the upper wall. The common form is both nototribe and didynamous, but I do not believe that a flower like Verbascum, with rotate corolla and exposed stamens, could develop either of these characters. Delpino regards Mentha as a degraded form of the Labiate type, and I am inclined to think that he is right.

Finally, for a discussion of zygomorphy from the standpoint of natural selection, and for a refutation of Henslow's view that floral organs must have varied simultaneously, see BOT. GAZ. xiii, 146, 203, 224.

R.

Some queer botany.

One runs across some funny botany in doctor books designed for home use! A few days ago I picked up a *vade mecum* of this sort written by an "M. D." who further styles himself "Licentiate of the Royal College of Physicians, Member of the Royal College of Surgeons (London)," with a lot more of high-sounding degrees. Here is what amused me. "*Podophyllum peltatum.* This plant, of the genus *Mandragora* [nothing like being scientific] has been supposed to be the same as that of which we read in the Scriptures as the mandrake. Its fruit, which is round and yellow, like a small orange, is very fragrant and luscious [mawkish, eaten by pigs and boys, *fide* A. Gr.] and is eaten in the East [wonder if that means "down east"] by women desirous of perfect health. The tuberous (?) root is the officinal portion." And this balderdash in a "sixth edition, thirty-third thousand"!

M. S. B.

Persian lilac on Weigela.

Last summer John Thorburn, LL. D., while visiting Yarmouth, Nova Scotia, discovered close to a house a bush of Japanese Weigela rosea on which there was a branch of Persian lilac carrying fine trusses of flowers. The specimens taken are now in our herbarium, and are undoubtedly as mentioned above. The lilac bushes grew at the back of the house and none where the Weigela grew. As Dr. Thorburn is one of our officers and a reliable gentlemen, I mention the circumstance as being noteworthy and solely on his authority.

JOHN MACOUN.

Ottawa, Canada.

Numbers of the Gazette Wanted.

The series of numbers making a nearly complete set of the BOTANICAL GAZETTE, which the editors have generously presented to the Marine Biological Laboratory at Wood's Holl, Mass., is such a valuable acquisition to our library, and is to benefit such a large number of persons that we are very desirous to fill out the set. Are there not among your readers some who can furnish to the laboratory as gifts or for purchase the lacking numbers? We require still: Vol. III, No. 10; Vol. VI, No. 9; Vol. VII, Nos. 8, 9, 11; Vol. VIII, No. 5; Vol. IX, Nos. 10, 11; Vol. X, Nos. 7, 8, 11, 12; Vol. XI, No. 1.

Boston. CHARLES S. MINOT.